

March 12, 1953

2/23/53
Dear Luca:

3/5/53!
Since receiving your letter of the 23d of February, I have waited about a week, expecting that you might write again in reply to my letter of the 21st. This was a mistake, as I have no doubt that this letter will now in fact cross yours in the mail, but it hope it will occasion no confusion if it is clear to you when I have received each of your letters, and written my own, and vice versa. My message of the 21st was not written as an answer to yours of the 23d, but it might as well. Let us agree, once and for all that we will proceed on the basis of your letter of the 23d, which is essentially the same as that stated before. I have nearly, but not entirely, recovered from the accumulated work that accompanied my return from Chamblee, and can begin to pay some closer attention to current matters. As you suggested, I have started to make some notes, using your superb Bull.W.H.O. paper as a basis, and will send these to you as soon as possible. As in this WHO article, the main emphasis should be given to a) the discriminatory tests of mechanisms of adaptation, and b) your recombinational studies. Such matters as transduction need be mentioned only in passing. I have some remarks in mind concerning Voureka's recent work, for which there may be some rational explanation. Since Hinshelwood (see Nature 2.7.53--I have only heard about it) no longer disregards mutation, perhaps he need not be mentioned at all. One other point: PAS has been claimed to retard the development of SM-resistance in TBC. Again, there is an obvious rational explanation. You mention you have written 3 papers. Which do you have in mind?-- WHO, Heredity? which else.

To answer some accumulated questions-- I am not sure what your frame of reference is in asking about coli serology? Are you referring to Kauffmann's work on the separation (more difficult than his book would imply) of H, K, and O antigens? Most of our ~~its~~ crossable stocks, as we now know, are rather poor material for serological studies that would be meaningful to the other coli-serologists. However, Esther has just now gotten some favorable hints on the crossability of occasional strains of the important serotypes, O55 and O111. The only comprehensive review of coli serology is Kauffmann's book: "Enterobacteriaceae", 1951, Munksgard, Copenhagen, and it is far from ideally satisfactory. He has also had a review some three or four years ago in Journal of Immunology.

Dr. Skaar's observations on the isolation of F- from 58-161 passed through galatin-agar motility medium seems to be confirmed. Spicer had so passed his 58-161 strain, in order to accentuate H-agglutinability, before storing it on Dorset egg medium, and the former undoubtedly accounts for the result. A collection of these strains is being sent. Of course, we are pursuing the question of mechanism for the phenomenon, but invite your participation in it.

Esther has just now been reading my letter of the 25th December (as the file is before us), and points out my error there, which explains the confusion about "P-G-F-". This is my own lapsus mentis. I meant, in that letter, W-1578 (which is M-F-lambda^S), in fact the immediate ancestor of W-1607. I was confused because W-1678 is another much used strain, P-G- but F+. Lately, however, I believe to have secured an F- derivative by the method of the previous paragraph, and will send this if true.

Nelson has finally succeeded in corroborating Hayes' sm effect, using Hfr. These experiments were started some long time ago, but it took some time to learn the best conditions for "killing" with SM. The survival ratio appears to vary over a 100-fold range, depending on the period for which the "killed" cells are incubated in buffer before plating. One can limit survivorship more drastically by longer treatments, but what "killing" means remains still doubtful.

FOLD SIDES OVER AND THEN FOLD BOTTOM UP AND SEAL.
OTHER ENVELOPE SHOULD BE USED.

However, the rate of recombination is still much less than for untreated Hfr. Treated F- are sterile. These studies may be less interesting than others using nitrofuracin, which may have a special toxic effect on the zygotes themselves, but there are numerous technical difficulties.

I have just learned that Hayes will be proceeding from CSH to spend some time with Delbruck at Caltech. This is no doubt the underlying reason for Delbruck's having invited Hayes to the CSH symposium.

There is little more to report at the instant, as the motilization-F experiments are in progress. I cannot refrain from mentioning, however, that a ~~specific~~ particular K-12 strain has been found to produce lambda with an incredibly high rate of transduction! About 1 lambda particle per thousand transduces Gal₄⁺ to competent Gal₄⁻ mutant strains. As each bacterium can adsorb several lambda particles, transduction frequencies of several percent of the bacteria have been noted. By a modification of the experiment, transduction of Gal⁻ to Gal⁺ strains has also been observed, and it is hoped we can now follow the history of individual transduced cells much more closely. ("Nft" is about 1 per 10⁶ lambda. Still, only Gal has been observed to be transduced in K-12).

I have heard nothing further from or concerning Chalef. As our accommodations will have to be planned during the next several weeks, and early communication would be appreciated.

In order to restore order to our correspondence, I will reply immediately to your next letter, if only to acknowledge it if its questions are anticipated here. It might be worthwhile to write explicitly the date of the last received letter, as I now add to the head of this one.

The likelihood of our travel this summer is still unsettled. Frankly, I would prefer to spend my own resources, if necessary, for a better occasion to travel in Europe (and to visit you) than the circus atmosphere of the Congresses. One can never tell whether there may be some windfall.

Your Heredity reprints, etc., just received. Thank you very much. Have you received your supply of our Genetics paper? They arrived here some time ago.

Please forgive my delay in sending the cultures: I thought you'd like to have these new F-, and I must spend some time reisolating H-310 and H-313.

I await your word also concerning the distribution of Hfr, so that I may reply to Hayes.

Yours, hurriedly,


Joshua